An experimental inquiry into the nature of relational goods, and their impact on co-operation

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An experimental inquiry into the nature of relational goods, and their impact on co-operation

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Abstract

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Abstract

Our experiment studies the impact of two types of relational goods on the voluntary contributions for the production of a public good, i.e. acquaintance among the contributors and performance of a team work before the experiment. Our results show that: 1) both team work and previous acquaintance increase the average contribution to the public good, and 2) there is a relevant gender effect with women contributing more or less than men, depending on the treatment. Therefore, we conclude that both relational goods are important to enhance co-operation, that acquaintance and working together are rather complements than substitutes, and that different relational goods produce different effects on cooperation. Also, we find further evidence for women's behaviour to be more context-specific than men's.

Keywords: relational goods; public goods; experiments; gender effect

1. Introduction

Among the relevant discoveries of the so-called economics of happiness is that *relational goods* (RGs) play a powerful role in determining the subjective well-being (SWB) of human beings. According to Becchetti et al. (2008, from the abstract), relational goods are "the affective/expressive, non-instrumental, side of interpersonal relationships": human relationships provide not only the results they are made for, but also a specific, additional good that stems from the enjoyment of the togetherness[1]. This is not surprising: a very short time elapsed since we were

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[1] The notion of relational goods is well rooted in economic literature, hence we will not pursue this discussion further. See mainly Becchetti et al. (2008, 2009b), Prinz and Bűrger (2009), Bartolini and Bilancini (2010), Bruni and Stanca (2008), Bartolini et al. (2007), and Pugno (2009). According to the first quotation, the economic analysis of relational goods started with Gui (1987). A detailed (albeit informal) definition may be found in Uhlaner (1989).

social animals to any effect, and it is highly plausible that we have not had time enough to adapt to the very recent individualistic society where most of us are living[2]. The more we enjoy RGs, the happier we are (or the greater is our SWB)[3]; hence RGs are normal goods. Probably they are not only final goods, but also *intermediate* goods, i.e. they are also part of the endowment of our *human capital*[4]. A person well endowed with RGs is happier than a person who is less endowed; but s/he also has more human relations, possibly a more productive attitude towards others, a better experience in dealing with people, and so on. Hence we may expect that RGs affect not only our well-being, but also our activities. But what exactly is a relational good? A Cro-Magnon fellow would probably be very surprised with some current definitions, that point towards "being together" irrespectively of what is done while together[5]. S/he dwelt in an environment where (simplifying) *everything* was made together. To disentangle *being* together from *doing* together would probably look quite convoluted and useless to her/him. It is reasonable to suppose that the feeling of "togetherness" we miss today, and which makes relational goods so precious, is strongly correlated with the *cooperation* that characterized (and characterizes) gathering-hunting societies, i.e with the collective accomplishment of some basic duty.

This hypothesis is highly coherent with a well-known criticism to the notion of *homo oeconomicus* (see, for instance, Strober, 1994; Ferber, 1995; Nelson, 1995), according to which

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[2] There is an increasing consensus that altruistic moods of human beings are rooted in our evolution as a social species. For a recent (and fascinating) discussion see De Waal (2013).

[3] Usually, the so-called economics of happiness adopts as dependent variable either the *happiness,* or the *life satisfaction,* or the *subjective well-being,* measured subjectively. This distinction is not relevant for our paper.

[4] For non-economists, it may be useful to remind that a normal good is a good for which there is a positive relation between income and quantity demanded. This definition here broadly intends to state the positive relation existing between quantity consumed of relational goods and SWB. An intermediate good is a good which is used as an input for the production of another good, either a partly finished or a final one. See the discussion of relational capital goods below in order to understand better the interpretation of RGs as intermediate goods.

[5] See for instance Gui and Stanca (2010, p. 109): "This framework has been developed with primary reference to encounters taking place within productive organisations or at their interfaces with counterparts in transactions. However, it lends itself to also analysing interpersonal relationships within families, informal social groups, or associations. Here the 'conventional output' to be taken into consideration may be, for instance: a home meal, the completion of a mountain hike, or the performance of an amateur music band. But the process of generation of relational outputs is basically the same". Analogously, Gui and Sugden (2005, p. 3) claim that "relational goods are the affective components of interpersonal relations [that] are usually perceived as having value through their sincerity or genuineness"; and Becchetti et al. (2009b, p. 2) that "relational goods [... are] the expressive/affective side of social interactions, for instance time spent with friends and family, in meetings of associations or communities etc.".

people are used to socialise into groups (family, friends, colleagues, etc.) and are used both to rely on and to feel responsible for the (relevant) others. According to this interpretation of the *homo oeconomicus*, people's actions can be expected to reflect their group identity and their differences in socialisation.

What has been discussed above provides support for the partition of relational goods into *relational consumption goods* and *relational capital goods* suggested by Uhlaner (1989) and more recently by Gui (2005) and Gui and Stanca (2010, p. 109), and is also the basis for the hypothesis that we will try to test in this paper[6]. The hypothesis is that there is a hierarchy in relational goods. To spend a night together playing cards or singing songs is less "relational" than spending a day working together to accomplish a relevant duty or to reach a common goal. Our concern will not be with the consumer good facet of different relational goods (how they affect our SWB), but with their facet as human capital goods; more precisely, with their effects on the propensity to cooperate. The hypothesis is that the endowment of relational goods affects that propensity positively. People linked by "weak" relational goods will cooperate less than people linked by "strong" ones[7]. In other terms, while we may expect that all kinds or RGs enhance cooperation, we may also expect that the effect may be greater or smaller according to their kind. The normative implications could be of some interest: if it is confirmed that working together is more effective than spending free time together, the enhancement of cooperative behaviours through the development of relational goods would require a promotion of cooperative work; if the opposite is true, the promotion of free-time institutions. Obviously the mix of the two could, in principle, be fine-tuned according to the relative weights of the two kinds of RGs (and possibly of other ones). We may attach a label to the possible

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[6] A detailed discussion of the distinction between the two kinds of relational goods is found in Gui (2005). He proposes to view interpersonal relations as 'encounters' – a broader concept than transaction and exchange – which consist of peculiar productive processes in which agents not only deliver ordinary goods or services, but also deliver relational outputs, distinguished in relational consumption goods (company, entertainment, recognition) and relational capital goods; these consist partly of 'information' about the parties who interact, and partly of 'affective bonds', for example, feelings of friendship (Gui and Stanca 2010). As Gui and Stanca point out, the stock of relational capital goods can be available for successive encounters with that same interacting parties, and also a decumulation of relational capital goods can possibly occur if interacting agents develop ill feelings towards each other (page 109).
[7] See the next section for a brief survey of the effects of cheap talk on cooperation.

extremes. The first one (only team work produces cooperation through a RG) can be defined the "Marxian" notion of RG: RGs are the manifestation of the common (or "class") consciousness of cooperating workers. The second one (being together produces cooperation) can be defined the "Catholic" one - all people are brothers (or sisters), and feel (or should feel) so irrespective of what they do or think, because they belong to the same church[8]. For our paper these labels are purely illustrative, but they help us to remember that the assessment of the role of RGs is likely to be crucial both for the Christian and for the socialist social philosophies. We will not pursue further this subject, but we think that it is sensible to call it into light to emphasize the richness of contents that characterizes the study of RGs.

Our approach is experimental. Basically, in our experiment we aim at studying the effects on the propensity to cooperate of two different kinds of RGs, that is *previous acquaintance* and *the performance of a collective task*. Section 2 reviews the related experimental literature, in particular the experimental literature on relational goods and public goods; section 3 illustrates the structure of the experiment and its implementation; after a digression on the gender effect in section 4, section 5 presents the results, and section 6 concludes.

2. Related experimental work

Experimental literature on relational goods is in its infancy. In their recent contribution Becchetti et al. (2011) use for the first time the concept of relational goods to explain cooperative behaviour in an experimental context. According to them, no other studies exist which introduce in an experiment the possibility of consuming RGs in order to study their impact on cooperation. Becchetti et al (2011) do so indirectly, by referring to the concept of RGs to interpret the results of two previous experiments that study the effect of the reduction of social distance on deviation from purely selfish behaviour in an Investment Game (Becchetti et al., 2007) and in a Traveller's

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[8] Actually, *Church* comes from the Latin *Ecclesia,* which in turn comes from the Greek word *Εκκλησία,* whose meaning is "general assembly."

Dilemma (Becchetti et al., 2009a). In both experiments a reduction of social distance - through removal of anonymity - is the consequence of a *voluntary* choice made by subjects to meet the counterpart after play. This generates "the possibility to consume relational goods through a personal encounter that agents will share after having interacted in the laboratory" (p. 27), therefore creating an experimental design which allows to study the effect of RGs on cooperation. The main results of the two experiments are then interpreted as the willingness to influence positively the disposition of the other players to the meeting, which is a crucial factor to create and consume relational goods during the encounter. The most significant result considered is that the willingness to consume RGs seems to increase the chance of detecting cooperative behaviour "even though it entails a monetary risk or a sure material sacrifice".

If we accept that the endowment of relational goods decreases if competition increases, some experiments on the effects of competition could be assumed to consider the effect of lacking of relational goods on that propensity. Brandts et al. (2009) study the effects of competitive rivalry on the disposition towards others in a social dilemma game without complete contracts. They find that rivalry impacts negatively on the behavioural disposition towards those interacted with, and decreases "subsequent willingness to help", potentially leading to the "obstruction of future cooperation" (p. 20). Besides, they find that: rivalry neither increases efficiency nor does it favour the short side of the exchange relation in terms of gains in earnings; it has a negative effect on the subjective well-being of those on the long side; it increases the well-being of those on the short side, therefore generating inequalities. A negative effect of competition on the propensity to cooperate has also been found by Canegallo et al. (2008) and by Carpenter (2005).

On the other hand, since the early experimental contributions (for instance, Smith, 1979, 1980; Marwell and Ames, 1981), experimental literature on the public goods (PGs) allocation problem has grown extensively in many different directions. A very consistent number of experimental works

studied (both in single and multiple-round experiments) the effects on contribution rates of different treatment factors, like the levels of the Marginal Per Capita Return (MPCR); the size of the players' group; the distinction between internal and external return; the existence of repetition with a fixed or a variable group; the opportunity to punish or reward the other players; the possibility for participants or experimenters to observe individual contributions; the introduction of pre-play communication (for references see surveys by Ledyard, 1995; Holt 2006; and works by Isaac et al., 1984; Isaac and Walker, 1988a, 1988b; Isaac et al., 1994; Goeree et al., 2002; Croson, 1996; Andreoni, 1988; Andreoni et al., 2003, among many others). Other experiments have studied variations in the payoff structure, by introducing a non-linear payoff function (see Laury and Holt, 2000 for a survey), or by requiring that total contributions exceed a specified threshold before any benefit can occur (see Bagnoli and McKee, 1991; Croson and Marks, 2000, Canegallo et al., 2008); the effects of other factors like culture, age and gender; the effect of altruism, of relative earnings and of attitude toward others (see Laury and Holt, 2000 for a survey).

Our implementation of the PGs problem follows the standard structure for research in this area, a game with multiple stages with a fixed group and constant MPCR, as introduced by Isaac et al. (1984). Since we adopt a form of pre-play communication, the experimental study of the effects of non-binding pre-play communication on free-riding behaviour is of particular relevance for this work.

The issue of communication has been addressed by many experimental works which have studied the impact of non-binding pre-play communication – cheap talk – on cooperative behaviour and efficiency, in both non-threshold and threshold environments (see Ledyard, 1995; Davis and Holt, 1993 for surveys). In non-threshold games – despite the theoretical prediction of no effect – many studies have found a significant increase in group payoffs in social dilemma experiments (Dawes et al., 1977) and in the level of contributions and efficiency in PGs (Isaac et al., 1985);

Isaac and Walker (1988b) notice that their results document the "impact of face-to-face communication in the reduction of free riding behavior" (p. 602), even when communication is costly (for this see the follow-up study by Isaac and Walker, 1991). Differently from the benchmark case, in presence of communication, repetition seems to increase cooperation (see Leyard, 1995 for a survey). More recently many authors have studied the impact of different forms of communication on cooperation and efficiency in different contexts (see among others Belianin and Novarese, 2005; Cason and Kahn, 1999; Bochet et al., 2006; Wilson and Sell, 1997; Güth et al., 2007). Their results show that previous communication does enhance cooperation; it is of interest that this finding is highly coherent with those obtained by psychologists about minimal groups, in the tradition of Tajfel (see f.i. Tajfel 1981)[9].

On the basis of all this research, we assume that pre-play communication does enhance cooperation; and consequently we adopt as a benchmark a cheap talk environment, to rule out the possibility that an apparent effect of relational goods be due to the cheap talk that unavoidably accompanies it.

Our paper is also related to the extant literature on group identity. In particular, some works have explored the effect on subjects of interacting with members of the same group. Eckel and Grossman (2005) engender different degrees of group identity using different procedures, for instance, by assigning people randomly to groups labelled with names of colour (e.g. blue, green, etc.). In this setting the experimental subjects did not know each other before. The authors find that "well-defined, cohesive teams may be more successful in deterring free-riding behaviour." (p. 384). Buchan et al. (2006) consider nationality: they assume that sharing the same nationality creates a kind of group identity and, thus, reduces social distance. Their results indicate that this type of group identity enhances other-regarding preferences, but they also find that this outcome is sensitive

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[9] Davis and Holt (1993, p.337) cast some doubts about this result, but only with reference to experimental settings different from the one adopted here.

to cultural traits specific to each nationality. Charness et al. (2007) also find that members of a group behave differently than non-members when group membership is salient, that is when they interact with each other and are observed by other members of the group. In particular members are more aggressive than non-members. Chen and Li (2009) initially induce group identity using preferences over a couple of paintings of different artists, then enhance identity with different procedures . Subjects are matched in groups according to their preferences for one painting rather than the other. Their results show that people are less likely to punish and more prone to reward members of one's own group than of the other one. Moreover, the authors observe that charity concerns increase within each group, while envy decreases. All these works induce group identity during (i.e. at the first stage of) the experiment.

To our knowledge, three papers may be compared more directly to ours. Brown-Kruse and Hummels (1993) and Cadsby and Maynes (1998) implement a design based on a threshold public goods game (TPGG), in which a sort of team work is present. A half of the subjects involved are requested to fill in a questionnaire before the experiment. The questionnaire "asks participants to share information about themselves. The goal […] was to engender a sense of membership in a group"[10]. The other half of the participants are randomly selected and play the TPGG without any previous treatment. Solow and Kirkwood (2002) test the effect of group identity in a basic (i.e. without threshold) public goods game (PGG). They select one half of the subjects "from a […] pool of undergraduates […] who had volunteered to take part in experiments generally" (p. 406) and the other half from the University of Iowa Hawkeye Marching Band. The former constitute the group of "strangers", the latter that of people having a group identity. In addition, half of the strangers filled in a questionnaire as in Brown-Kruse and Hummels (1993) and in Cadsby and Maynes (1998), while the other half just played the PGG.

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[10] Cadsby and Maynes (1998, p. 608).

Our design differs from those used in the works quoted above in two fundamental aspects. The first is that our cheap talk is not finalised to the accomplishment of any duty, and does not concern the players' strategies, as it is usual in the public goods literature with cheap talk; instead, it is simply left open to the subjects[11]. The second difference is in how we build the group identity: we do not rely on the mutual exchange of personal information nor on something in between a hobby and a job (like playing in the Band), but rather on a task which is very closely related to the type of studies followed by the subjects and looks like a possible job. Moreover, we also exploit group identity existing before the experiment, recruiting also subjects who were already acquainted.

3. The experimental design and implementation

The experiment was conducted by the Laboratorio di Economia Sperimentale e Simulativa of the University of Piemonte Orientale at the Faculty of Economics of the University of Torino. A total of 128 undergraduate students, 64 females and 64 males, took part in 32 sessions, 8 for each of the 4 treatments. Females and males were equally distributed across the sessions and the treatments; the groups were made of subjects of the same gender, to avoid possible chivalry (see Eckel and Grossman, 2001) or jealousy effects[12]. The computerized experiment was conducted by means of a Z-tree program.

The game. In each session 4 subjects of the same gender were asked to play 10 repetitions of the following standard version of a public goods game (PGG). In each repetition each subject was endowed with 60 experimental monetary units (EMU) of the value of €0.01 each, and s/he had to decide whether to keep them or to allocate all or part of them to a common fund. The total amount of the EMU allocated to the fund were doubled and divided in equal parts to all participants,

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[11] Note that in this paper the locution "cheap talk" has a meaning slightly different from the traditional one. As we noted, it usually refers to informal talking about the game strategies, while here the topic of the talk is not constrained in any way.

[12] Of course, this is not a neutral choice, as the composition of the group matters, but at least it ensures that all the groups have the same variance (0) as regards to their gender composition, and therefore our results cannot be biased by differences in the sex composition of the groups. On this see also Iredale et al. (2008).

irrespective of how much each one had contributed to the fund. Every EMU which was not allocated to the common fund remained wholly to the participant as earnings, while for each EMU that the participant allocated to the fund - and for each EMU allocated to the fund by another participant- s/he kept as earnings 0.5 EMU. The total earnings in a session were given by the sum of what was earned in each of the 10 repetitions. No participation fee was paid.

In this standard version of PGG the unique sub-game perfect Nash equilibrium predicts always to contribute nothing to the common fund (perfect free-riding), while the Pareto-efficient solution predicts allocation to the fund of all the initial endowment (perfect cooperation).

The treatments. The experimental set-up consisted of four different treatments which combined the following different features:1) the subjects knew each other when registering to the session, in which case it was required in the invitation to register together as a group for the same session; 2) the subjects did not know each other, in which case they registered to the experiment individually; 3) before playing the PGG the subjects had either to perform a team work or 4) to engage in a cheap talk. In the team work treatment the subjects were given the balance sheets of three different companies and had to perform a budget analysis task, by calculating the Return on Sales (ROS), Return on Investment (ROI) and Return on Equity (ROE) ratios, and the cash flow margins for each company. They were asked to write down a short report on the basis of their calculations. The task lasted about 45-50 minutes. In the cheap talk treatment the subjects were gathered in a separate room and left together to chat. As mentioned above, differently from other experiments no hint was given to the subjects about the topic of their conversation. When the cheap talk was used in other experiments, the subjects were instructed to talk about the game they were going to play. Of course this procedure might influence the results of the game, since the subjects may share some strategy, may think of the possible equilibrium of the game etc. Moreover, in the real world friends talk about a number of subjects, and any conversation about a public goods game is very unlikely. In

order to mirror a real "cheap talk" situation as much as possible, we decided not to give any instruction to the subjects in the room, but, rather, we left them free to talk about any subject of their choice. Of course, it is possible that less talkative subjects interacted less than the others, but this mirrors again the real world[13]. In summary, we have the following four different treatments: 1) Previous acquaintance and team work, 2) previous acquaintance and cheap talk, 3) no previous acquaintance and team work, 4) no previous acquaintance and cheap talk; each treatment played by 4 groups of 4 male and 4 groups of 4 female subjects. As explained in the previous section, the cheap talk treatment is the benchmark for the assessment of the role of the two relational goods[14].

To avoid misunderstandings, it is useful at this point to emphasize that we assume the presence of cheap talk as the benchmark, while what we aim to compare are the effects of acquaintance and common work. The reason why we introduced cheap talk is that, as we saw, it enhances cooperation, so that in absence of it, it would be impossible to disentangle the effect of common work (that implies cheap talk) from the effect of cheap talk itself.

At the beginning of each session the subjects gathered in front of the computer room. In the team work treatment they were asked to sit together around a single computer terminal in the centre of the room, which was available to them to consult any material they might have needed to complete the task. In the cheap talk treatment they were taken to a different small room (with no computers) and asked to wait while the computer room was set and were conducted back to the computer room after 20 minutes[15].

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[13] Following a popular stereotype, some could think that in such a cheap talk women talked more than men. However, several studies have found no evidence for this to be true (see for example Scott, 1980 and Cameron, 2010).

[14] The condition of previous acquaintance may correspond to different degrees of acquaintance. Our dichotomy - presence or absence of acquaintance - relies on the theory of minimal groups, according to which the *assignment* to a group is sufficient to create an in-group feeling (see f.i. Brown, 1986, p. 543 and ff.; Tajfel, 1981).

[15] The different duration of the team work and of the cheap talk should create no concern. There is no reason why a work session should last as a chatting one. We wanted to adopt realistic timings. Consequently we opted for a working time analogous to that of a University lecture and for a talking time analogous to that of a break between two lectures. We planned to interrupt the team work after one hour, but the case never produced.

Once the team work or the cheap talk was over, the subjects were sat at the four computer terminals at the extreme corners of the room, so that no communication was possible. At the beginning of the experiment the instructions appeared sequentially on each participant's computer screen and were read aloud by the experimenter at the same pace; the instructions would proceed to the following page only when all the four participants had clicked on the 'Continue' button on the screen.

The PGG game described above was illustrated to the subjects. It was made clear to them that their total earnings would be the sum of the earnings in each repetition and that they would be paid at the end of the experiment under the usual conditions of anonymity. It was highlighted that all the initial sum would be kept as earnings in case no EMU was allocated to the common fund and that the total earnings in case all participants allocated all the sum to the fund would be 120 EMU. After the description a trial round was run, making clear that the choice in this round would not contribute to the final earnings. After this a written summary of the instructions was distributed to the participants, and the real experiment started.

In each repetition the subjects faced a screen with an empty box where they had to enter the amount of EMU they wanted to allocate to the common fund. It was reminded to them that they had 60 EMU to allocate, and 2 minutes to take their decision, write the number and click the 'Save' button; after that time they would earn nothing on that repetition[16]. After each subject had taken the decision or the time allocated had passed, a new screen for a new repetition appeared. In every repetition each subject could see summarized in a table the total amount of the common fund (but not the amount allocated by each subject), her/his earnings for that repetition, the amount of EMU kept and the division of the common fund and her/his total profit up to that repetition in EMU and euros. At the end of the 10 repetitions the total profit (in EMU and euros) appeared on the screen.

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[16] The case never occurred.

Once the experiment was over, the subjects were asked to leave the room and come back individually to be paid and fill in their receipts.

4. A digression on the role of gender effects.

Among the number of results obtained by the experimental literature, one appears to be common to (almost) all the designs and settings: gender matters for choices. Croson and Gneezy (2009) sum up this individuating three macro-results: 1) women are more risk averse than men, 2) women and men are not differently socially oriented, but the social preferences of the former are more malleable than those of the latter; 3) women are more averse to competition than men. A public goods game, as the one that we use in this work, involves a certain level of risk and is based on pro-social attitudes. However difference (1) narrows as education and labour income increase (Croson and Gneezy, 2009); while our subjects are undergraduate students, and have no significant labour income, they have a high level of education, which should therefore attenuate the effect of risk. As for effect (2), Nowell and Tinkler (1994), Rooney et al. (2005) and the survey by Croson and Gneezy (2009) find that women are generally more generous than men, and that this holds also in public goods games. This general finding is consistent with Gilligan (1982), who claims that women think of social problems in terms of care and responsibility in relationships, whilst men emphasise the role of rights and rules. Indeed, on this psychological basis it is straightforward to expect that, in a public goods game, women contribute more than men. However, while males' behaviour does not react significantly to different designs and contexts, that of females does (Croson and Gneezy, 2009). Gilligan (1982) and Croson and Gneezy (2009) suggest also that, while women should contribute more, they might respond quicker than males to the context and thus converge faster than males to the equilibrium (namely free riding); since cooperation and free riding are mutually exclusive, in reality it is not possible to cast any hypothesis on women's behaviour *ex ante*. In particular women's generosity is more evident in mixed-gender than in single-gender (female only) groups. This last result, however, does not appear to be robust across different games

and contexts. Brown-Kruse and Hummels (1993) find the opposite effect: in their experiment (based on a PG game) females contributed significantly less than males; Cadsby and Maynes (1998) find no gender effect in a replication of Brown-Kruse and Hummels' design as well as in a threshold public goods experiment, and Solow and Kirkwood (2002) obtain the same results in a standard PG game. Dufwenberg and Muren (2006) find that people tend to be more cooperative with persons of their own than of the opposite sex, and this outcome is stronger among women than among men. Croson and Gneezy (2009, p. 16) suggest also that "women are more sensitive [than men] to social cues in determining appropriate behavior" and conclude that women's behaviour is much more context specific than men's. In a public goods experiment, we should thus expect that the choices of the female participants will be more path-dependent than males', i.e. women will retaliate more than men against behaviours that signal poor cooperation, while they will respond to cooperation more cooperatively than men. Nevertheless the literature also suggests that the presence of greed would result detrimental for cooperation more among men than among women (Simpson, 2003; Kuwabara, 2005), i.e. the level of cooperation of women in a scarcely cooperative environment is generally higher than that of men in an analogous situation.

A major element of context specificity is the gender composition of the group of players; in particular females tend to be very sensitive to this aspect and to condition their behaviour on it (Cadsby et al., 2010; Charness and Rustichini, 2011). There is evidence that both men and women condition their behaviour (and thus their choices) on the gender of their opponent(s), but the effect is stronger among women than among men. Andreoni and Vesterlund (2001) find that men and women are alternatively more altruistic, depending on the price of giving: as it grows, females become more altruistic than males. However this is in contrast with Cox and Deck (2006), who find that women are more generous than men when "the costs of generous behaviour are low" [...] and "there is an absence of reciprocal motivation" (p. 597). Although the quoted works are not based on public goods games, they suggest 1) the presence of a gender effect when giving and reciprocity are

involved and 2) the uncertainty of the direction of the gender effect. Eventually Charness and Rustichini (2011) find women to cooperate more when they are observed by in-group members than when out-group members observe them. This suggests that female players tend to cooperate less with strangers than with people they are already acquainted with.

As the extant literature on the gender effect is not conclusive, it is not possible to cast hypotheses *a priori*; however it is likely that also in our experiment gender will play some role, and therefore, in order to be able to control this better, we decided to form homogeneous gender groups of participants, as discussed in the previous section[17].

5. Results.

We focused mainly on three variables, i.e. the individual contribution to the common fund and the two possible extreme behaviours, perfect cooperation and perfect free riding. In each repetition we define a player a "perfect co-operator" when he contributes the entire endowment of 60 experimental monetary units (EMU) to the mutual fund, whereas a participant who keeps all (i.e. contributes 0 to the fund) is defined a "perfect free rider". Basically this entails that these two behaviours are represented by a couple of dummies, which take value 1 if the participant has displayed the relative behaviour and 0 otherwise.

Since the subjects play the PGG matched in groups of four for ten consecutive rounds, a dependence occurs between the individuals within each group from the second round on. In an attempt to control for this interdependence, we added two variables to capture both the interdependence within the groups and the reaction to the average behaviour of the other

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[17] Incidentally, let us note that one might argue that our sample suffers another gender-related selection bias, as a typical BA female student of economics could be more aggressive than a male one, because of the presence of sexist stereotypes. An answer to this concern comes from Gerdes and Gränsmark (2010), who, studying the world of chess players at professional level (i.e. a male-dominated environment), find that this type of selection does not produce outcomes significantly different from those of other environments considered in the literature.

components[18]. The first (named $\Delta C_{i,t-1}pos$) captures the difference between one's contribution and the average contribution in the previous round, when this is positive; the second variable (called $\Delta C_{i,t-1}neg$) captures the same difference as before, but when it is negative. Operationally, they are computed as follows:

$$\Delta C_{i,t-1}pos = \begin{cases} \dots \\ \dots \end{cases}$$

$$\Delta C_{i,t-1}neg = \begin{cases} \dots \\ \dots \end{cases}$$

where $C_{i,t-1}$ is the contribution of the i-th player at round t-1. The introduction of these two variables tries to compensate for the presence of interdependence by making it captured by the two differences. Since each subject reacts to the choices of his group only, and since the effect of the others' contributions in round t-1 affects one's choice in round t, then the two variables that represent the difference of one's contribution with respect to the mean of the group should capture this effect[19]. The other controls are the gender of the respondent, the treatment variables and the individual gain in the previous round. The impact of the treatments, of the player's gender, of the two delta variables and of individual gain on the absolute contribution is analysed by panel tobit, and by panel probit[20] regressions for the two extreme behaviours, as will be illustrated in more detail below[21].

We first summarise the main results in five graphs, which report the average contributions per round either by treatment or by gender. At the bottom of each graph, we also present a table which

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[18] These variables are calculated following Chaudhuri et al. (2006), Canegallo et al. (2008), Ottone and Ponzano (2010) and Lotito et al. (2013).

[19] Unfortunately the method adopted here does not dissolve the covariance between the choices within each group. This entails inflated standard errors for the observed coefficients, what may have rendered apparently non significant some coefficients that are in fact significant. An alternative option to the method could have been a clustered tobit regression. However this would have sensibly reduced the number of observations, rendering the estimates too weak and scarcely reliable. A second alternative (considering the results observed in the first rounds only) would have incurred the same problem.

[20] The distribution of the errors of the estimates is actually not normal, nor logistic. However, graphical tests show that it approaches a normal distribution rather than a logistic. As a consequence, the results were estimated through a panel probit regression rather than through a logistic regression.

[21] For all estimations, STATA 10 was used.

highlights whether the series of data presented in the respective figure are significant[22]. In each table we report three tests: first we test whether the series in each couple are statistically different on average. Then, since this is a raw measure of significance, we refine the analysis by testing whether the series are statistically significant in the first and in the last five rounds (out of ten) of the PGG. This allows for showing that the behaviours of the two groups compared in each figure are (or are not) statistically different not only during the whole game, but also in each half of it. In particular Figure 1 presents the average contributions of the players who either had previous acquaintance or had not. We can notice that previous acquaintance has two effects: first the contributions in the first round are higher for those who were already acquainted than for the others; and second, while in the first group the average contribution remains almost stable, in the second sub-sample it decreases, as usually observed in past experiments.

Figure 2 depicts the average contributions of people who either performed the team work or did not. It is interesting to see that, while there is no difference in the first round, the contributions of the team workers tend to decrease a bit more slowly than those of the others. Indeed, they are statistically different, on average, in the second half of the game.

The presence of a possible gender effect is analysed through Figure 3. Here it is interesting to note that, while the males start contributing slightly more than the females, the latter tend to maintain cooperation, whereas the former's contributions show a negatively-sloped trend. We must highlight that the treatments were actually four, and that the first two figures combine each two of them. Econometrics will provide a more detailed analysis. The tests reported in the table under the figure reveal that the contributions are statistically different in both the first and the second half of the game. However, since the difference between the two groups changes sign around the fifth round, the mean contributions computed over all the ten rounds are not statistically significant.

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[22] We have preferred to add these tables instead of displaying confidence intervals in the graphs, as these would have rendered the figures difficult to read.

Figure 4 shows the relevance of performing the team work among the acquainted. While the level of contribution is high, the subjects who worked in team display no decrease in the average contribution, while the others' gradually diminishes between the first and the fourth round, and then remains almost stable. This suggests that working in team helps to maintain cooperation among the acquainted. Figure 5 reports the same effect among non-acquainted. The relevance of acquaintance is of stunning evidence: not only the contribution in the first round is much larger among the acquainted, but the others also display a relevant decrease in the average contributions.

Eventually, as we claimed the presence of some gender effect, we present graphical evidence of it, across the different treatments. Figures 6 and 7 disentangle the four treatments and the two genders and suggest the presence of some mixed gender effect[23]. In particular Figure 6 suggests that working in team helps to maintain cooperation among non-acquainted males (grey lines in Figure 6): although these two sub-samples start from the same average contribution in round 1, the decline is steeper for those who did not worked in team. The picture is analogous (black lines) for the acquainted males, although after the eighth round the distance between the averages narrows. Eventually the graph confirms that acquaintance enhances cooperation. Taken together these results suggest that acquaintance and team working are complements for males. Figure 7 compares four sub-samples built in the same way for the female group. Here the evidence is mixed. On the one hand acquaintance seems to play a strong and big role for the subjects who worked in team. In spite of the different starting points, we observe that: first, cooperation increases between the acquainted members of the team, whilst it slightly decreases between the team workers who were not acquainted; second, the lack of acquaintance does not seem to have any effect for the subjects who did not work in team; third, the team work appears to affect the level (but not the maintenance) of

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[23] This means that each point of the graph represents the mean of four contributions.

cooperation between the acquainted females, but it seems to be detrimental for cooperation between those who were not already acquainted before the experiment.

Since the game involved ten rounds, the data are analysed also through a panel tobit regression, censoring at the two extremes of the range of the possible contributions to the common fund (0 and 60 EMUs). Since we limited the endowment of the subjects at 60 EMUs, tobit technique seems to be appropriate. Indeed some of the subjects could have wanted to contribute more (especially after hoarding some money), but they were prevented from doing so. Therefore – although the maximum contribution did not represent exactly their choice – nevertheless they chose it. This concentrates the probabilities of all the choices equal to or greater than 60 EMUs in a unique point (namely 60 EMUs). The tobit technique allows for censoring the observations at the ceiling value (and also at the floor, should also this be artificially imposed) and corrects the variance-covariance matrix and the estimates accounting for this load in one point.

The results of the tobit regressions are reported in Table 1, where two models and three samples are considered. The first model includes only the gender and the treatment variables as controls, while in the second also the individual gain in the previous round and the two "delta" variables defined above are included. The three samples are: the whole sample, the male and the female sub-samples. The strong gender effect detected in the previous analyses justifies such a choice.

We can notice that previous acquaintance has a positive and strong effect in both the models and all the sub-samples, while working in team enhances contributions only for males, for whom the effects of the two relational goods are very similar. As it is often found in the extant literature, we also notice that the more a contributor gained in the previous round, the more s/he contributes in the next. The coefficients of the two "delta" variables are interesting: in either case (i.e. whenever one's bid in round x − 1 differs from the others' mean) the effect on deviating from the others' mean in a

round has a positive effect on one's contribution in the next. Although our data do not allow for testing this hypothesis, a possible interpretation for this result is that people retrieve some utility from knowing that they "did better than the others".

Public games allow for cooperation and for free riding. Free riding in our experiment is analysed in Table 2. The dependent variable is a dummy which takes value 1 any time a player contributes 0 to the mutual fund. The table thus presents panel probit estimates. Only results related to the marginal effects are displayed.

Previous acquaintance and team work have the expected sign (negative) and in model 1 are significant for both the whole sample and the male sub-sample. Among women only the acquaintance has a significant influence, suggesting that the results for the whole sample are driven by the males (although also for them the effect is only weakly significant). Indeed this is not surprising, as we have already highlighted that women without previous acquaintance react in an opposite way than males under the team work treatment. The magnitude of the marginal effects confirms the results of the graphical analysis: while both the team work and the acquaintance have a positive influence on cooperation, the latter is stronger than the former. It must be noted that the effect of team working is not robust to the introduction of controls other than gender and the treatment variables. However this loss of significance is not unexpected and does not hinder the previous results, given that the treatment influences the contributions of the players to the common fund (as shown in Table 1). The two "delta variables" indicate here the consistency of one's behaviour over time: indeed people who contributed less than the others' mean in round x − 1 are more likely to free ride in round x, and players who contributed more than the others' average in round x − 1 are less likely to free ride in round x.

The last table presents the results for the behaviour opposite to the previous one: here the dependent variable is a dummy capturing perfect cooperation, i.e. the contribution of the full endowment. It takes value 1 if the players puts 60 EMU in the mutual fund and 0 otherwise. Here, while previous acquaintance plays a relevant and strong role in promoting full cooperation, (especially in model 1, and likely for the same reason explained above) this is no longer the case for working in team. This has only some weak significant effect in the male sub-sample. Previous acquaintance appears to be a strong preventer of free riding, and an enhancer of full cooperation, while working in team is less effective in this sense. An analysis of contributions in the first round, reveals that the players acquainted before the experiment contributed on average more than those who were strangers. This is not the case when team workers are compared to those who were not requested to perform the team task. The information provided by the two "delta variables" in this case is analogous to that for the previous case of free-riding and shows once more consistency in the players' behaviours. These are interesting results, as they reinforce the effect of the treatments: their effect on the subjects' behaviour is indeed structural, in the sense that they modify the average behaviour (as measured by the average level of the contribution), while the intrinsic propensity to contribute remains unchanged. In other words: people who would on average contribute less (or more) than the average behave consistently with this intrinsic characteristic of their personality, but their average contribution is affected by the treatment. This suggests that the forms of socialisation studied in this paper are of particular interest, as, although they are not able to modify the relative-to-others behaviour of an individual, they are able to act on (and to modify) the average level of the contribution.

One might argue that "previous acquaintance" has a broad meaning, which may include different degrees of acquaintance. In particular, if the subjects got acquainted working together in the past, then it is possible that the treatments "acquainted with cheap talk" and "non-acquainted with team work" are somewhat equivalent. Our data do not contain any information about how the subjects

got acquainted. However, we observe the contributions of acquainted both under the "cheap talk" and under the "team work" treatments, and the latter contributed on average 33% more than the former (this difference is significant at 99% level). This means that, however the subjects got acquainted, working in team has a positive and statistically significant effect on the contributions to the public good.

6. Conclusions

Our results suggest that: 1) relational goods do enhance cooperation; 2) both team work and previous acquaintance increase the average contribution to the public good, although the second appears to be stronger than the first; 3) acquaintance and team work are rather complements than substitutes; 4) there is a relevant gender effect, the women contribute more or less than men, depending on the treatment, what provides further evidence that the women's behaviour is more context-specific than men's; and 5) different relational goods have different effects on the propensity to cooperate. It is of interest that result 4 is coherent with some previous findings (see also section 2 above). Brown-Kruse and Hummels (1993) and Cadsby and Maynes (1998) find no difference between questionnaire and strangers groups overall: although the females who filled-in the questionnaire start contributing significantly less to the public good than all the other groups, yet they end up with contributions that are statistically equal to the others'. Solow and Kirkwood (2002) show that the mean contributions are the same for both the strangers and the questionnaire subjects; furthermore the average contribution of the males from the Band is significantly larger than that of stranger and questionnaire males, while no significant effect is detectable between different groups of females (although those who filled in the questionnaire contributed less than the strangers and those from the Band).

Result 5 also deserves a further comment. We found that team work is more effective than cheap talk (with one exception), but obviously we cannot generalize this results to the real world. The case

of non-acquainted females indicates that there may even be cases where the team work is detrimental. Nonetheless, our experiment supports the hypothesis that the specific nature of the relational goods is relevant.

To sum up, we may resort to a biological metaphor to suggest quite safely that the "relational goods" are more a family (or a gender) than a species. It should be of great interest to study the specific patterns of the different species.

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Figure 1. Average contributions to the fund per round and by acquaintance
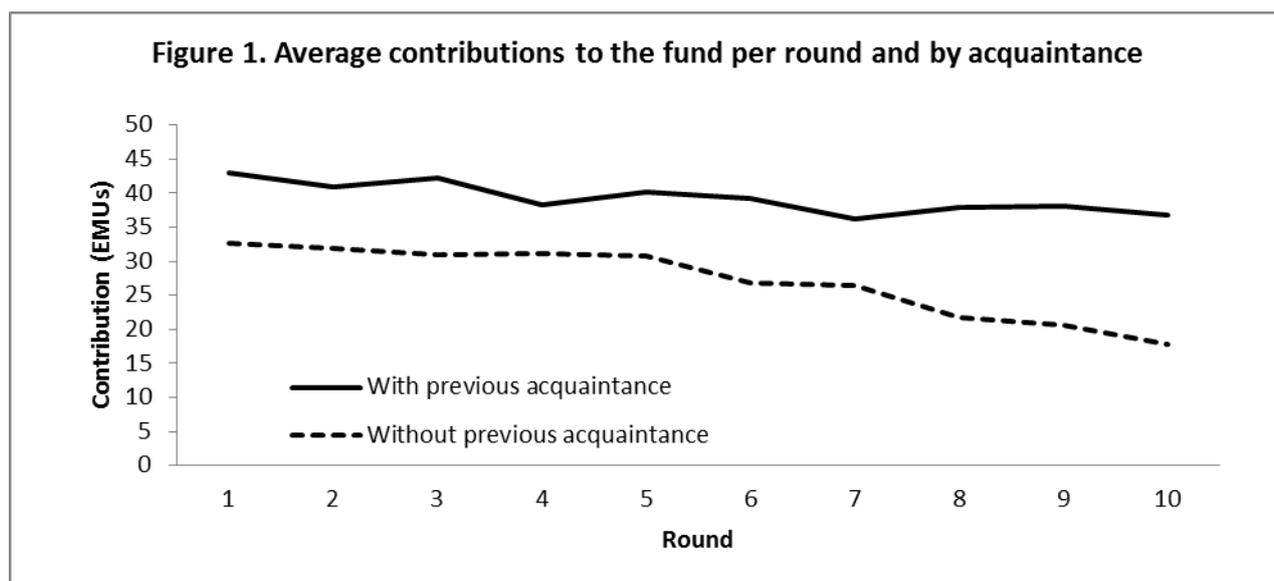
Average contributions with and without previous acquaintance (whole sample - s.e. in brackets)

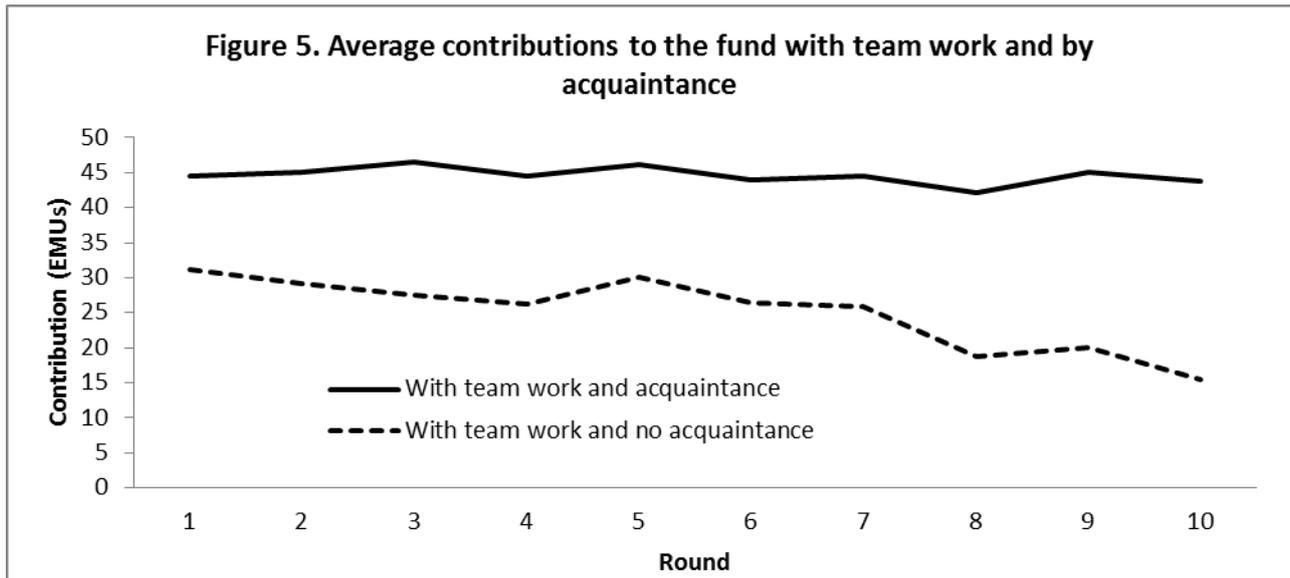|	With acquaintance	Without acquaintance	Significance	Observations
Average contribution in all the 10 rounds	39.29	27.08	***	1280
	(0.889)	(0.959)		
Average contribution in the first 5 rounds	40.91	31.48	***	640
	(1.207)	(1.340)		
Average contribution in the last 5 rounds	37.67	22.67	***	640
	(1.302)	(1.330)		

Note: the figures in the table are average EMUs contributed to the common fund (public good). The significance of the differences was computed through a t-test using STATA 10. The significance levels are: *** 99%; ** 95%; * 90%.

Figure 2. Average contributions per round and by team work
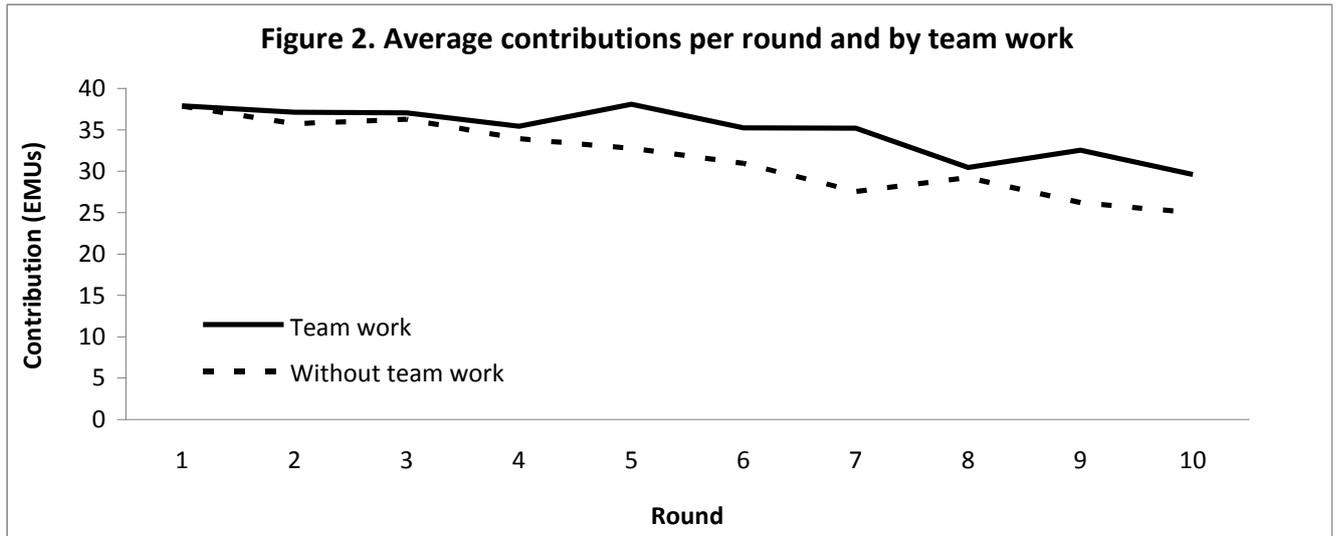
Average contributions with and without team work (whole sample - s.e. in brackets)

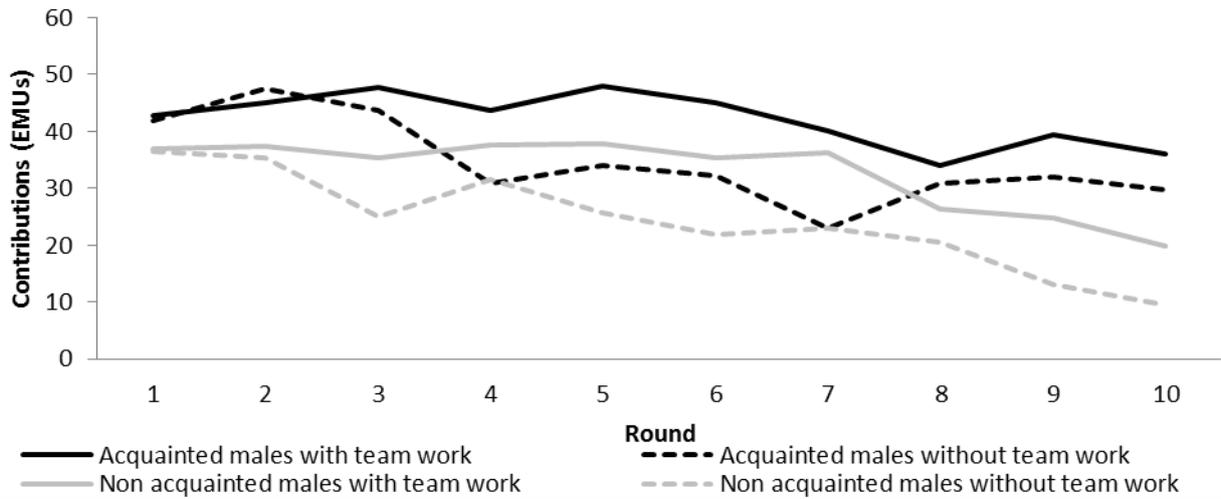|	With team work	Without team work	Significance	Observations
Average contribution in all the 10 rounds	34.84	31.53	***	1280
	(0.974)	(0.933)		
Average contribution in the first 5 rounds	37.09	35.30	-	640
	(1.307)	(1.296)		
Average contribution in the last 5 rounds	32.58	27.76	***	640
	(1.434)	(1.313)		

Note: the figures in the table are average EMUs contributed to the common fund (public good). The significance of the differences was computed through a t-test using STATA 10. The significance levels are: *** 99%; ** 95%; * 90%.

Figure 3. Average contributions to the fund per round and by gender
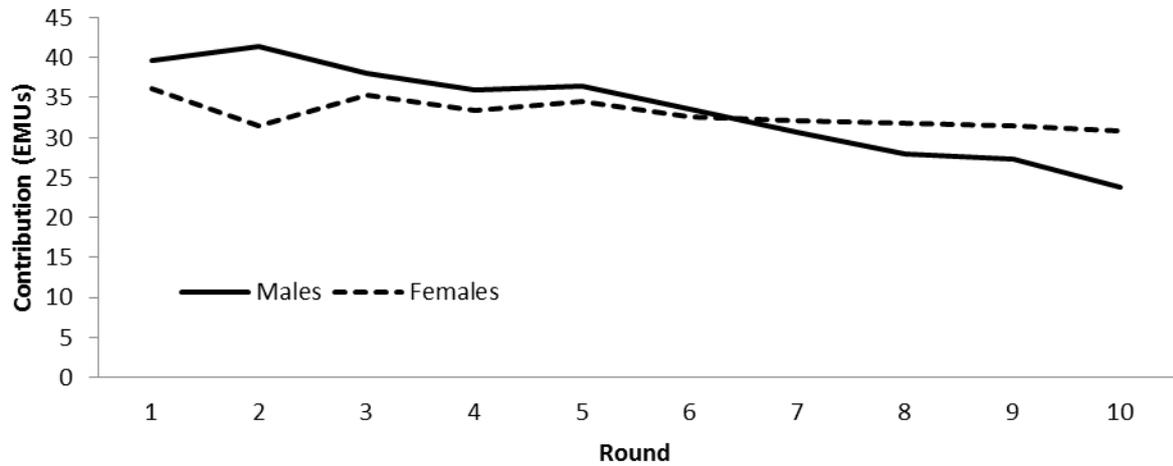
Average contributions by gender (whole sample - s.e. in brackets)				
	Males	Females	Significance	Observations
Average contribution in all the 10 rounds	33.42	32.94	-	1280
	(0.942)	(0.970)		
Average contribution in the first 5 rounds	38.22	34.17	***	640
	(1.258)	(1.336)		
Average contribution in the last 5 rounds	28.62	31.72	**	640
	(1.352)	(1.405)		

Note: the figures in the table are average EMUs contributed to the common fund (public good). The significance of the differences was computed through a t-test using STATA 10. The significance levels are: *** 99%; ** 95%; * 90%.
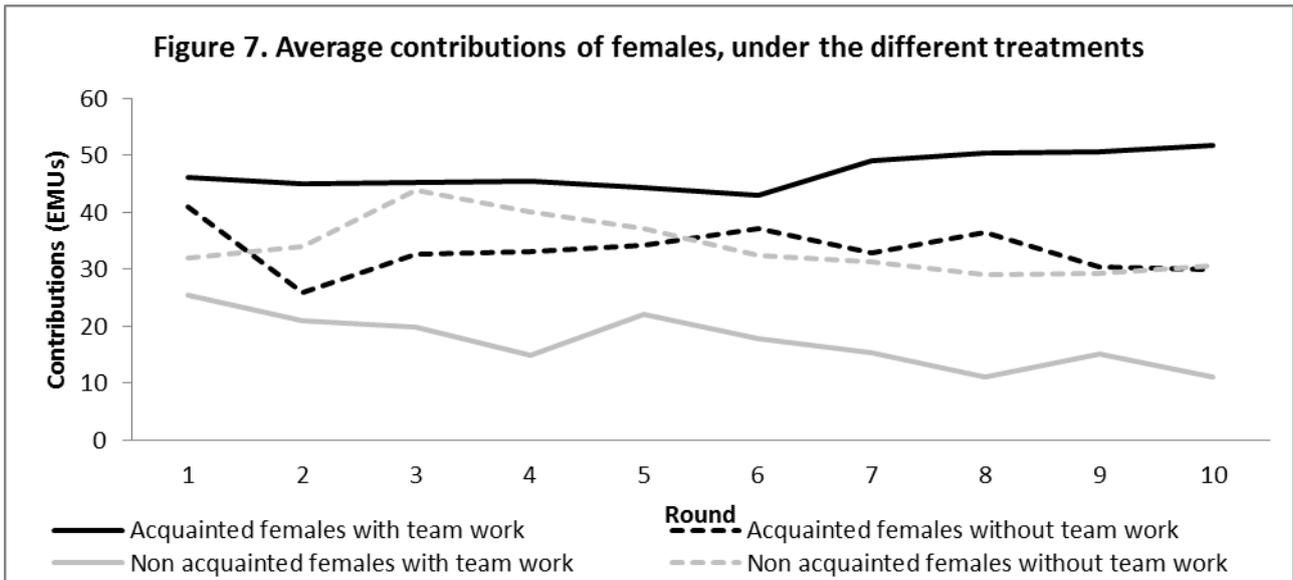
Figure 4. Average contributions per round, with acquaintance and by team work
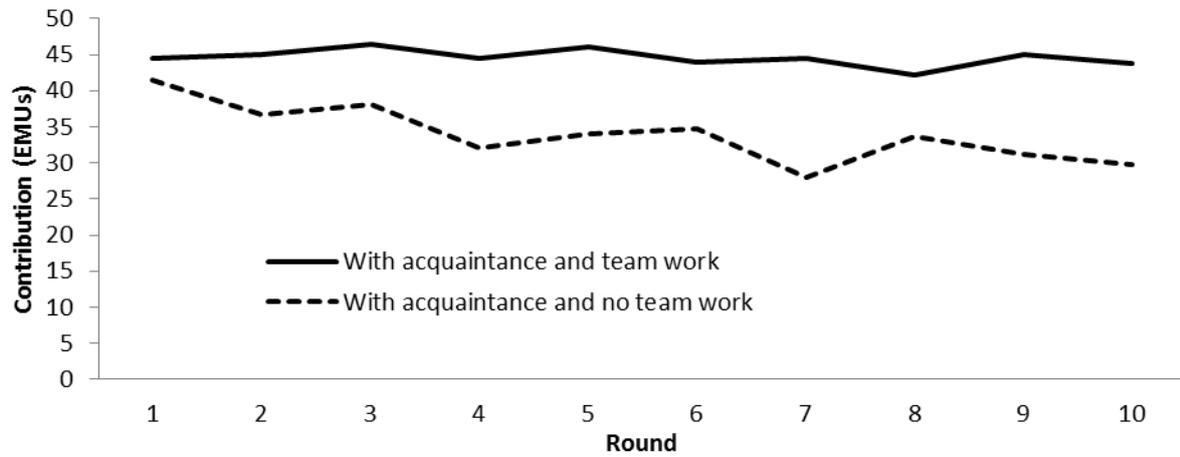
Average contributions for acquainted subjects with or without team work (s.e. in brackets)				
	With team work	Without team work	Significance	Observations
Average contribution in all the 10 rounds	44.62	33.95	***	640
	(1.198)	(1.245)		
Average contribution in the first 5 rounds	45.35	36.47	***	320
	(1.612)	(1.732)		
Average contribution in the last 5 rounds	43.89	31.44	***	320
	(1.774)	(1.779)		

Note: the figures in the table are average EMUs contributed to the common fund (public good). The significance of the differences was computed through a t-test using STATA 10. The significance levels are: *** 99%; ** 95%; * 90%.

Figure 5. Average contributions to the fund with team work and by acquaintance

Average contributions for subjects who worked in tean with or without previous acquaintance (s.e. in brackets)				
	With acquaintance	Without acquaintance	Significance	Observations
Average contribution in all the 10 rounds	44.62	25.05	***	640
	(1.198)	(1.328)		
Average contribution in the first 5 rounds	45.35	28.83	***	320
	(1.612)	(1.844)		
Average contribution in the last 5 rounds	41.89	21.27	***	320
	(1.775)	(1.869)		

Note: the figures in the table are average EMUs contributed to the common fund (public good). The significance of the differences was computed through a t-test using STATA 10. The significance levels are: *** 99%; ** 95%; * 90%.

Figure 6. Average contributions of males under the different treatments

Average contributions for acquainted males with or without team work (s.e. in brackets)				
	With team work	Without team work	Significance	Observations
Average contribution in all the 10 rounds	42.14	34.57	***	320
	(1.757)	(1.717)		
Average contribution in the first 5 rounds	45.44	39.59	***	160
	(2.212)	(2.297)		
Average contribution in the last 5 rounds	38.84	29.55	***	160
	(2.695)	(2.439)		
Average contributions for non-acquainted males with or without team work (s.e. in brackets)				
	With team work	Without team work	Significance	Observations
Average contribution in all the 10 rounds	32.76	24.22	***	320
	(1.990)	(1.801)		
Average contribution in the first 5 rounds	37.02	30.85	**	160
	(2.670)	(2.617)		
Average contribution in the last 5 rounds	28.50	17.59	***	160
	(2.889)	(2.255)		

Note: the figures in the table are average EMUs contributed to the common fund (public good). The significance of the differences was computed through a t-test using STATA 10. The significance levels are: *** 99%; ** 95%; * 90%.

Figure 7. Average contributions of females, under the different treatments

Average contributions for acquainted females with or without team work (s.e. in brackets)				
	With team work	Without team work	Significance	Observations
Average contribution in all the 10 rounds	47.11	33.34	***	320
	(1.609)	(1.814)		
Average contribution in the first 5 rounds	45.26	33.35	***	160
	(2.360)	(2.559)		
Average contribution in the last 5 rounds	48.95	33.32	***	160
	(2.218)	(2.588)		
Average contributions for non-acquainted females with or without team work (s.e. in brackets)				
	With team work	Without team work	Significance	Observations
Average contribution in all the 10 rounds	17.34	33.99	***	320
	(1.538)	(2.018)		
Average contribution in the first 5 rounds	20.64	37.41	***	160
	(2.205)	(2.801)		
Average contribution in the last 5 rounds	14.04	30.56	***	160
	(2.094)	(2.873)		

Note: the figures in the table are average EMUs contributed to the common fund (public good). The significance of the differences was computed through a t-test using STATA 10. The significance levels are: *** 99%; ** 95%; * 90%.

Table 1. Effect of previous acquaintance and team work on individual contributions (s.e. between brackets)

	Model 1			Model 2		
	Whole sample	Males	Females	Whole sample	Males	Females
Male	-3.090			-0.892		
	(5.438)			(2.097)		
Team work	8.547	15.802	0.988	3.027	5.269	-0.796
	(5.444)	(6.432)***	(8.803)	(2.089)	(2.028)***	(4.179)
Previous acquaintance	20.545	16.457	24.361	8.582	5.593	12.4016
	(5.451)***	(6.429)***	(8.823)***	(2.102)***	(2.024)***	(4.209)***
Delta_positive				1.122	1.186	1.094
				(0.031)***	(0.044)***	(0.041)***
Delta_negative				1.040	1.103	0.921
				(0.026)***	(0.031)***	(0.041)***
Gain in the previous round				52.489	62.999	41.204
				(1.583)***	(2.098)***	(2.203)***
Constant	29.467	24.499	31.855	-17.247	-27.926	-6.797
	(5.430)***	(5.500)***	(7.620)***	(2.464)***	(2.472)***	(4.048)*
Observations	1,280	640	640	1,152	576	576
Log-likelihood	-4,209.71	-2,201.26	-2,003.66	-2,944.38	-1,489.65	-1,419.30
Wald chi	16.62	12.34	7.62	4,150.18	2,812.63	1,141.75

* significant at 90% level; ** significant at 95% level; *** significant at 99% level.
Tobit panel regression (lower truncation = 0; upper truncation = 60)

Table 2. Effects on free riding (probit panel estimates; standard errors in brackets)

	Whole sample	Males	Females	Whole sample	Males	Females
	Marginal effects	Marginal effects	Marginal effects	Marginal effects	Marginal effects	Marginal effects
Male	-0.053			0.058		
	(0.230)			(0.205)		
Team work	-0.401	-0.619	-0.169	-0.305	-0.369	-0.162
	(0.233)*	(0.298)**	(0.356)	(0.207)	(0.269)	(0.314)
Previous acquaintance	-0.974	-1.102	-0.820	-0.681	-0.805	-0.539
	(0.236)***	(0.306)***	(0.358)**	(0.212)***	(0.267)***	(0.320)*
Delta_neg				0.032	0.035	0.028
				(0.008)***	(0.012)***	(0.011)***
Delta_pos				-0.026	-0.035	-0.020
				(0.008)***	(0.012)***	(0.010)**
Gain in the previous round				-2.858	-3.343	-2.358
				(0.378)***	(0.575)***	(0.515)***
Observations	1280	640	640	1152	576	576
Log-likelihhod	-430.79	-213.13	-216.22	-367.92	-178.89	-186.57

* significant at 90% level; ** significant at 95% level; *** significant at 99% level.

Table 3. Effects on full cooperation (probit panel estimates; standard errors in brackets)

	Whole sample	Males	Females	Whole sample	Males	Females
	Marginal effects	Marginal effects	Marginal effects	Marginal effects	Marginal effect s	Marginal effects
Male	-0.212			-0.273		
	(0.324)			(0.242)		
Team work	0.507	0.862	0.068	0.360	0.613	0.166
	(0.325)	(0.351)***	(0.585)	(0.244)	(0.293)**	(0.346)
Previous acquaintance	0.955	0.737	1.221	0.572	0.636	0.228
	(0.327)***	(0.352)**	(0.592)**	(0.251)**	(0.295)**	(0.367)
Delta_neg				-0.042	-0.028	-0.077
				(0.008)***	(0.010)***	(0.014)***
Delta_pos				0.029	0.030	0.040
				(0.009)***	(0.012)***	(0.012)***
Gain in the previous round				3.320	2.575	5.207
				(0.457)***	(0.537)***	(0.800)***
Observations	1280	640	640	1152	576	576
Log-likelihhod	-533.99	-292.51	-237.27	-441.94	-245.20	-189.22

* significant at 90% level; ** significant at 95% level; *** significant at 99% level.

Response to Reviewer 1

          Dear Referee,

Thank you very much for your comments. We have carefully read them and we have tried to do our best to improve the paper following your suggestions. Please notice that some changes that you did not request were made following another referee's suggestions.

          In order to make easier for you to follow our responses to your comments, we have reported them in this letter. Your comments are in black, our answers in blue.

```
Reviewer #1: Probit is used when the response variable is a frequency or counts
and independent variables are mostly nonb-matric which is equivalant to the
Factor ANOVA, except in Factor ANOVA the dependent variables is matric. The
output in this paper does not match the PROBIT output that I have used in the
past.You could have used Logistic Regression since your responce is binary (0
and 1) and you can have a mixture of matric and non-matric variables. Also I
like to know what software was used to analyse your data.
```

          Thank you for this comment. Indeed we did not mention the econometric package used for the analyses. We have now added this information in footnote 21. The fact that you are used to analyse data with SPSS may explain the difference in the appearance of the output. Actually we use probit estimation in the paper, as this seems the best estimation strategy for dichotomous variables. The logistic regression entails that the errors of the estimates are distributed as a logistic, while probit is preferable when these have a Gauss' bell distribution. The distributions of these errors in our case is close to the Gauss' bell, and therefore we have opted for probit estimates. This is also discussed in footnote 20.

Response to Reviewer 2

Dear Referee,

Thank you very much for your comments. We have carefully read them and we have tried to do our best to improve the paper following your suggestions. Please notice that some changes that you did not request were made following another referee's suggestions.

In order to make it easier for you to follow our responses to your comments, we have reported them in this letter. Your comments are in black, our answers in blue. Overall we think that your comments and your suggestions have contributed to improve the clarity, the readability and, more in general, the quality of the paper.

```
Reviewer #2: In this paper, the authors examined the effect of previous
acquaintance and team work, on cooperation through a lab experiment. The authors
found that previous acquaintance was stronger than working together in
increasing contributions to public goods, and complemented each other in
producing this effect. Moreover, males and females contributed more or less than
each other depending on the treatment conditions. My comments on the strengths
and weaknesses of the paper are as follows:

Strengths:
- The paper has a compelling introduction. The proposed examination of whether
"being together" and "working together" in the past (the two types of relational
goods) have different  effects on cooperation is interesting. I have never seen
any studies examining similar relationships before and I agree with the authors
on the strong practical implication of the findings.
- The paper structure is clear and easy to follow. I compliment the authors on
making an attempt to focus their literature review on justifying their proposed
hypotheses and experiment design.
```

We thank you for highlighting these strengths and for appreciating the paper in general.

```
Points for improvements:
- The authors should provide a clear definition of "relational goods" at the
begining of the introduction. It will make it easier for the readers to follow
subsequent arguments in the remaning of the introduction.
```

Thank you for this comment. We have now strengthened the definition of what relational goods are, also adding a quotation from the paper by Becchetti et al. (2008) in the main text. Additionally we have also modified footnote 1 making a more explicit reference to the literature on the subject. These changes should make the paper clearer and help the reader to follow it.

Reference
Becchetti L., Pelloni, A., Rossetti, F., 2008. Relational goods, sociability, and happiness. *Kyklos* 61(3), 343-363.

```
- The first few sentences of the introduction do not sound clear to me, as
follows:
   "It is not surprising, a very short time elapsed since we were social animals
to any effect, and it is highly plausible that ..." -> what does "we were social
animals to any effect" mean?
```

We have now clarified this point, adding footnote 2. In addition we have added a reference to De Waal (2013), who proposes a fascinating discussion on the issue.

Reference
De Waal, F. (2013), *The Bonobo and the Atheist: in Search of Humanism among Primates*. New York: Norton.

```
    o "The more more we enjoy RGs, the happier we are (or the greater is our
SWB); hence RGs are normal goods. Probably they are not only final goods, but
also intermidiate goods..." -> What do the authors mean by "normal goods" and
"intermidiate goods" ? Also, I could not see the connection between these three
statements.
```

Thank you for asking for clarification here. Indeed it was missed in the paper. Footnote 4 now explains the economic meaning of normal goods and intermediate goods and tries to clarify the connection between the statements.

```
- The terms "relational consumption goods" and "relational capital goods" need
more clarification.
```

Thank you. We have added footnote 6 to explain this better. In particular, here we refer to Gui (2005) and to Gui and Stanca (2010), who outline very well this distinction.

References
Gui, B., 2005. From transactions to encounter: the joint generation of relational goods and conventional values. In Gui and Sugden (eds). *Economics and Social Interaction.* Cambridge: Cambridge: University Press.

Gui, B., Stanca, L., 2010. Happiness and relational goods: well-being and interpersonal relations in the economic sphere. *International Review of Economics* 57 (2), 105-118.

```
- In the last paragraph of the section "Related experimental work", the authors
said they had manipulated the "cheaptalk" condition differently from previous
research. The authors need to justify why they manipulated it the way they did
and how it was better than the previous study.
```

Thank you for suggesting this clarification. In the description of the experimental design, we now explain the reasons of our choice. In particular, in the past experiments, the subjects were required to talk about the game. In other words, the game was explained before the cheap talk, and the experimenters asked the participants to discuss the strategies before playing. Such a treatment is likely to induce uncontrollable biases in the experiment. Indeed the experimenters are not aware of the contents of the discussion, since, in order to let the subjects free to talk, they are not present in the room during the talk. This entails the impossibility of knowing the effect of the talk on the outcomes of the game. Moreover, it is also possible that some groups agree in playing the best strategy, whereas others do not or do not identify the best strategy at all. Eventually (and perhaps more importantly) real cheap talks between friends generally do not focus on game theory and strategic behavior. As we are interested in relational goods, then we tried to implement a treatment that mirrored the real world as much as possible.

```
- From the introduction, I understood that the authors wanted to examine these
causal relationships: (1) influence of "being together in the past" on
"cooperation" and (2) influence of "working together in the past" on
"cooperation" and (3) their interaction effect.  In the experiment, "being
together in the past" was operationalized as being acquaintances, and "working
together in the past" was operationalized as a pre-experiment joint work on a
task. I concern that being accquantances might include co-worker relationship,
```

and therefore blurs the distinction between the two constructs. Consequently,
the authors' conclusion that previous acquaintance has stronger effect on
cooperation than previous team work might reduce to longer co-worker
relationship has stronger effect on cooperation than shorter co-worker
relationship. In that case, the finding is not as interesting as the proposed
hypotheses in the introduction.

We agree with the referee; past acquaintance may include past co-working. However, our sample is randomly selected, and therefore past co-workers should be present in all the groups. Moreover, previous acquainted people are subjected to both treatments. Also assuming that previous acquaintance involves past co-working, the effect of any past co-working would be present both in the group treated with the cheap talk and in the group treated with the teamwork. Therefore, any possible effect of previous co-working, being present in both groups, should not bias the results. In particular, the effect of the team working on the acquainted is similar to the effect of team working on the non-acquainted. This outcome supports our previous claim that any possible effect of past co-working between the acquainted is likely not to have affected the experimental results. We discuss this at the end of the Results section.

- In fact, in my opinion, the experiment would be much better if the "being
together" were operationalized as whether the subjects have pre-experiement
cheaptalk, and the subjects' acquaintance before the experiment were treated as
a variable to control for.

We respectfully disagree on this point. Our first reason is that, as explained before, the randomness of the selection of the subjects and the randomness of their assignment to the different treatments should clean the results. The second reason is that the econometric analysis actually allows for controlling for these two effects so that we can isolate the contribution of each treatment. In other words, in the econometric estimations the acquaintance before the experiment is treated as a variable to control for, as you require. Then, the results of the econometric estimation allow for reading the effect of team working "neat" of that of previous acquaintance and *vice versa*.

- I wonder why the authors manipulated the "cheaptalk" condition by letting the
subjects simply sitting in the same room. The problem of this treatment is that
level of interaction among the subjects might well influence the experiment
outcome. I noticed from the results that the "female, cheaptalk" group
contributed more to public goods than the "female, teamwork" group, while the
"male, cheaptalk" group contributed less to public goods than the "male,
teamwork" group. I wonder if it was because the females talked more than the
males did in the "cheaptalk" period, not really because of their gender
difference.

Thank you for this comment. Indeed the extant literature on the issue does not find any gender difference in how much people are talkative. However, in order to dissolve doubts about this traditional stereotype, we explain it in footnote 13 and support our position referring to Scott (1980) and Cameron (2010).

References
Cameron, Deborah, 2010. "Sex/gender, language and the new biologism" *Applied Linguistics*, 31(2): 173 – 192.

Scott, Kathryn P. 1980. "Perception of communication competence: what's good for the goose is not good for the gander" *Women's Studies International Quarterly*, 3(2-3): 199 – 208.

- I am not clear whether the observed differences across treatments (i.e. the graphs) were statistically significant.

We agree on this point. Indeed the graph did not show confidence intervals. However, since the introduction of these in the graphs would render them hard to read, we have added a table at the bottom of each graph. These tables test: 1) whether the contributions are statistically different from each other over each entire series of ten rounds; 2) whether the contributions are statistically different from each other over the first five rounds and 3) whether the contributions are statistically different from each other over the last five rounds. Although this method is less precise than displaying confidence intervals, it represents a good compromise between the sake of clarity and completeness of information and the readability of the figures. Additionally, the statistical significance of these differences is also corroborated by the econometric analyses presented in the paper.

- I am not familiar with the panel tobit regression, and therefore withhold my comments on this analysis. However, I would suggest the authors to add a paragraph or two on explaining this technique to the unfamiliar audience.

Thank you for this suggestion. We have now added some explanation of this technique in section 5 of the results. We believe that this will help unfamiliar readers to understand the main features of the estimation techniques, the reasons why it was chosen, and its strengths.